Fostering Opportunities

Results of a Randomized Controlled Trial

REPORT HIGHLIGHTS:

- **Fostering Opportunities is the only proven practice in Colorado that improves educational outcomes for middle and high school students in foster care.**
- **Within one year of having access to Fostering Opportunities, students' attendance and behavior at school improved (i.e., fewer suspension incidents).**
- **Within two years of having access to Fostering Opportunities, students' rate of passing their classes improved.**
- **More research is needed to determine if Fostering Opportunities impacts high school graduation rates. Descriptive results suggest that within one year, there was a 50.76% increase in the number of high school students who were on track to graduate.**

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Report Number: 18-105D. Date: February 2024

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Acknowledgements

Thank you to the leadership and program staff at Jefferson County Public Schools and Jefferson County Human Services who work tirelessly to bring the Fostering Opportunities program to life. The launch of this four-year pilot program was a Pay for Success project to align child welfare and education systems to support the educational success of youth in foster care and is a collaborative effort among the Fostering Opportunities Governance Committee, with representatives from the State of Colorado, Jefferson County Public Schools, Jefferson County Human Services, and the Colorado Evaluation and Action Lab (Colorado Lab). The delivery of this intervention was funded by the Community First Foundation and the Governor's Office of State Planning and Budgeting under a Pay for Success contract with the State of Colorado and Jefferson County School District R-1. Prior to the Pay for Success pilot, Casey Family Programs invested in the initial development of this program. After the conclusion of the Pay for Success Pilot program, the intervention—which is now a proven practice—is being sustained through the Foster Care Success Act (House Bill 22-1374).

This research was supported by Arnold Ventures (formerly the Laura and John Arnold Foundation). The opinions expressed are those of the authors and do not represent the views of Arnold Ventures, the State of Colorado, Jefferson County School District, or other project partners. Policy and budget recommendations are the opinions of the Colorado Lab authors and do not represent the budget or legislative agendas of state agencies, the Governor's Office, or other partners. Any requests for funding or statutory changes will be developed in collaboration with the Governor's Office and communicated to the legislature through the regular budget and legislative processes.

Data Sources

The study uses school-level student data from Jefferson County Public School District R-1 and Jefferson County Human Services.

Suggested Citation

Clemens, E.V., & Belcher, C. (February 2024). *Fostering Opportunities: Results of a randomized controlled trial.* (Report No. 18-105D). Denver, CO: Colorado Evaluation and Action Lab at the University of Denver.

Introduction

Fostering Opportunities is an innovative student engagement program for middle school and high school students who have experienced foster care. The program is:

- Delivered by one or more education agencies working in close partnership with local child welfare agencies.
- Designed to be responsive to changes in participating students' schools, living situations, caregivers, eligibility for services, and child welfare case status.
- Designed to consider the network of people and systems (within and beyond education and child welfare) that are important to each student's attendance, behavior, course completion, and engagement in school.
- Designed to provide continuity in supports and services for as long as students need a dedicated mentor and advocate to be successful in school.

Education agencies take the lead on service delivery because eligibility for the program continues beyond the closure of child welfare cases. Students with a history of foster care often need trauma-informed educational support and mentoring throughout their entire K-12 educational experience. The goal of the program is to help youth who have experienced foster care be successful in school and earn a high school credential.

A Model for Innovation and Evidence-Building

Fostering Opportunities was developed because of the clear need to improve high school graduation rates for students in foster care. The percentage of youth in foster care who graduate with their class varies throughout the nation, but in Colorado, the four-year graduation rate for students in out-of-home placement in the class of 2022 was 30.0%.[1, 2, 3, 4, 5]

Most interventions aimed at the goal of improving the graduation rates of students who have experienced foster care are spearheaded by child welfare agencies or the judicial system, and for this reason, services tend to end when students exit the foster care system.[6] A student's risk for adverse educational outcomes does not end when they return home or are adopted. In fact, there is some evidence to suggest that the risk for educational outcomes may be elevated after a removal episode ends.[7] Education agencies are uniquely positioned to serve these young people even after their foster care case closes.

Fostering Opportunities was developed through a local education and child welfare partnership. It supports students until they graduate or demonstrate a natural system of academic support. It follows the students through planned and unplanned school changes.

With the goal of supporting the unique population of students in foster care, Fostering Opportunities was conceptualized, developed, and piloted in Jefferson County (Jeffco), Colorado, a geographically diverse 890-square-mile area that is home to a significant number of youths who experience foster care. This local education and human services partnership learned from promising practices in other states (e.g., Treehouse in Washington State, Students in School Rule! In Ohio[8, 9]) and evidence-based student engagement programs that are not foster-care specific such as *Check & Connect.*[10] Fostering Opportunities emphasizes social capital theory as the program's theoretical bedrock because of the unique, systems-level needs of students in foster care, above and beyond other marginalized student populations.

The Five Steps to Building Evidence: Moved Fostering Opportunities from a "Theory-informed" to a "Proven Practice"

The Colorado Steps to Building Evidence model is a five-step process that has been adopted by the Governor's Office of State Planning and Budgeting for use when considering budget requests.

Step 1: Program Design (Manual Published July 2019). The Colorado Evaluation and Action Lab (Colorado Lab) in partnership with Jeffco Public Schools and Jeffco Human Services developed the *Fostering Opportunities Program Manual: Middle and High School Version*.

Step 2: Identify Outputs (January 2019 through December 2020). Five key process benchmarks were monitored and reported to the Pay for Success (PFS) Governance Committee on a quarterly basis for the first two years of the project. By the end of Year 2, the program met or exceeded all five implementation benchmarks. Program implementation fidelity was also assessed in depth in March 2020 and March 2021, demonstrating strong adherence to the Fostering Opportunities model both prior to and during the COVID-19 pandemic.

Steps 3 and 4: Assess Outcomes and Attain Initial Evidence (Report Published May 2021). A preliminary outcomes report described performance of the Fostering Opportunities pilot after four semesters of program delivery. Descriptive comparisons between treatment and control groups indicated a greater than 10% improvement in the number of suspension incidents. Although the program was delivered with fidelity during the pandemic, changes in how attendance was recorded, and grading practices likely influenced the initial findings related to those outcomes. At this point in the project, outcomes were assessed for all study participants regardless of how long they had been enrolled in the study. Some study participants had been enrolled for one semester; others had been enrolled for up to four semesters.

Step 5: Attain Causal Evidence (This Report): This report details the key findings and results from the randomized controlled trial (RCT). Results described the impact of the Fostering Opportunities intervention at one year and two years after randomization into the study. It is an intent-to-treat evaluation, meaning that the students in the treatment group were offered the option of enrolling in the Fostering Opportunities intervention. *Note: Initial results of this RCT were reported in September 2022 with this same sample at the conclusion of the PFS pilot contract period in order to measure* "success" *of this PFS pilot program*.

Looking Forward: Program Expansion and Future Evidence-Building Activities

The steps to building evidence are intended to be an iterative process. This academic year (2023-2024), two new school districts began administering the Fostering Opportunities program: Denver Public Schools and 27J Schools, a district northeast of the Denver metropolitan area. As implementation begins at these sites, resourced by the Foster Care Success Act (House Bill [HB] 22-1374), evidence-building will initially focus on Steps 1, 2, and 3, revising program materials to capture new innovations, identifying outputs to ensure sites are delivering the program with fidelity, and comparing outcomes for students served to baseline data.

Description of the Study

This report presents findings from an analysis of the Fostering Opportunities program as it was administered in Jeffco. The purpose was to estimate the impacts of the Fostering Opportunities intervention on school attendance rates, course pass rates, suspensions, and being on track for high school graduation at one and two years after randomization.

The study was an RCT where sixth to 12th grade students who were in foster care (at entry into the study) were randomly assigned to either the Fostering Opportunities intervention ("treatment") or business as usual ("control") condition. The study followed an intent-to-treat model, meaning outcomes were evaluated based on the offered service. Therefore, all students who were enrolled in the study were included in the analysis, regardless of their level of engagement in the intervention. We used outcome data from one and two calendar years after randomization into the study.

Research Questions:

1. **What is the impact of Fostering Opportunities on students'**
   a. **attendance rate,**
   b. **course pass rate,**
   c. **odds of being suspended,**
   d. **number of times being suspended among those students who were suspended at least once, and**
   e. **being on track for high school graduation**

at one and two years after randomization?

The intervention was implemented by "specialists" hired by the Jeffco school district who check in weekly with students, ensure caregivers and child welfare case workers have timely and accurate information about students' educational progress, and consult with teachers on trauma-informed approaches to help the students be successful in school. These specialists follow students through planned and unplanned school changes within Jeffco schools and to adjacent school districts. The intervention and the study design assume that some students will transfer out of the school district, and procedures are in place to continue some aspects of service delivery and to track student outcomes.

Fostering Opportunities Video

Want to learn more about the program and hear from students? Watch this five-minute video.

Key Findings

Fostering Opportunities significantly improved educational outcomes for students who experienced foster care.

> **Access to the Fostering Opportunities program improved attendance rates within one year and were sustained at the two-year follow-up.**
>
> **Access to the Fostering Opportunities program decreased frequency of suspension incidents, among those students who were suspended at least once within one year and this effect was sustained at the two-year follow-up.**
>
> **By the end of two years, students who had access to Fostering Opportunities were passing more classes on average than the control group.**

Students' attendance and behavior typically need to improve before their academic performance increases. Practically, it is difficult to pass classes when students are simply not in the classroom due to attendance issues or suspensions. Thus, it makes sense that it would take longer to affect change in students' rate of passing their classes. In this study, at the end of one year of having access to the Fostering Opportunities program, students' rate of passing courses was not significantly different than those who were in the control group. However, when measured at the end of two years, students who had access to Fostering Opportunities passed more courses on average than their peers who did not have access to the program. All courses during the study period are "counted" in the analysis, meaning that the positive two-year outcomes are inclusive of both years that students had access to the program.

Fostering Opportunities is responsive to students' changing needs.

Fostering Opportunities is an intensive student engagement intervention, that by design allows specialists to decrease the intensity of the intervention (e.g., number of check-ins per month) as students become more stable academically and demonstrate self-advocacy skills and a natural system of support. This step-down approach is referred to in the program manual as "active monitoring." If or when students need more support, the program intensity can ramp back up in response to the students' changing needs. The findings that attendance, behavior, and course pass rate were all significantly better for students who had access to Fostering Opportunities at two years after randomization suggests that this step-down approach is working, and initial gains are being sustained.

This study did not have enough students in high school to determine if Fostering Opportunities impacts graduation rates.

> **There were only 95 students enrolled in the study who were in high school for at least a full academic year. This means there were not enough students to determine with confidence that any observed differences between the treatment and control group can be attributed to having access to the intervention.**[i]

[i] With the current sample of 95 high school students, even with a relatively large effect size (0.3), this research question only achieved a power of 0.29, well below the benchmark of 0.8. To reach a power of 0.8, maintaining the same proportion of students in the treatment and control groups and assuming the same effect size, we would have required a sample of 360 students. Each power analysis assumed an alpha of .05.

Descriptive results suggest that Fostering Opportunities holds promise for impacting high school graduation rates:

- **44.26% of students who had access to Fostering Opportunities were on track to graduate at one year after they had access to the program, whereas 29.41% of the students in the control group were on track to graduate.**

Descriptively, this suggests there may be a 50.49% increase in students being on track to graduate, but additional research is needed to confirm these results.

The total sample for this study included students enrolled in Grades 6-12 at randomization into the program. The outcome of "on track to graduate" is only assessed for students who were in high school during the first year after randomization. Thus, the sample size for this outcome analysis was particularly small and there were not enough observations to determine if the observed difference between the treatment and control groups were attributable to having access to Fostering Opportunities or if they occurred by chance.

Scaling this Proven Practice is Urgent

Students in the CONTROL GROUP were doing worse in school two years later than they were at baseline (i.e., start of the study), suggesting an urgency to scale this proven practice.

Descriptive analyses suggest that students who do not have access to Fostering Opportunities are typically doing worse in school each year that they do not have this type of support. Attendance and course pass rates were lower one and two years later than at baseline. This is consistent with prior research that indicated students who experience foster care typically do not make a year's worth of academic progress in a year's time (i.e., growth).[11] This underscores the importance of supporting children when they initially enter the child welfare system to buffer the foster care achievement gap.

Implications

- Evaluate the feasibility of scaling Fostering Opportunities to other school districts.
- Then, consider scaling Fostering Opportunities to all districts that demonstrate readiness to implement the program.
- Then, relaunch an RCT to build additional evidence of effectiveness.

This RCT was conducted in the school district that developed the intervention. The education and child welfare leaders in this geographic area were champions for the program. The supervisor for Fostering Opportunities specialists was instrumental in developing and refining the intervention. Thus, this RCT was much like a laboratory setting.

Evaluating the feasibility of scaling Fostering Opportunities to other school districts is a precursor to additional investments in scaling. The Colorado Department of Human Services has contracted with the Colorado Lab to evaluate the Fostering Opportunities programs that are resourced under HB22-1374. The evaluation plan will focus on feasibility (e.g., acceptability of the program in new geographic areas), fidelity monitoring, and outcomes tracking. Fidelity monitoring will help inform continuous quality

improvement and determine when to track outcomes of the program. Outcomes evaluation will be used to determine if students in other districts have attendance, suspension, and course pass rates that are similar to students who had access to the program in Jeffco.

From an evidence-building perspective, once there is evidence that Fostering Opportunities can be scaled successfully to new districts and students' outcomes are tracking as expected, then the program is ready for scaling to all districts that demonstrate readiness to implement it. The Colorado Implementation Science Unit developed site readiness protocols to gather information of readiness of new geographical areas to implement the program. These protocols can also help inform supports that could be provided to help prepare interested school districts and their child welfare agency partners to implement Fostering Opportunities with fidelity.

When there are school districts that are implementing Fostering Opportunities with fidelity and have a wait list (i.e., more eligible students than specialists can serve) an RCT can be launched to build additional evidence for program effectiveness. This may help move Fostering Opportunities to a new or higher designation by the Title IV-E Prevention Services Clearinghouse, which has benefits for Family First resources. Randomizing students into the program can make access to the program more equitable; each student has the same chance of getting into the program.

Methods

Intent-to-Treat Randomized Controlled Trial

Students in Grades 6-12 who were in foster care at entry into the study and enrolled in Jeffco Public Schools were randomly assigned to having access to the Fostering Opportunities intervention ("treatment") or business as usual ("control") condition. Randomization occurred at the start of each semester using a computer-generated random number. Sibling pairs were randomized by alternating the random assignment based on the lowest grade and the highest grade of the sibling group. Randomization weights, set on a per cohort (semester) basis, ranged from 0.3 to 0.8 probability of assignment to treatment.

> **Random assignment procedures were followed with fidelity. There was no indication of crossover.**

The randomization procedures were piloted during the building period, fall of 2018. By the time the study launched in the spring of 2019, the process and clear paths of communication and timelines with the providers and data contributors were established. There has been no indication of crossover since the study launched.

The cut-off dates for being part of a cohort were as follows:

- Students randomized between August 1 and October 1 were included in the fall cohort.
- Students randomized by February 15 were included in the spring cohort.

Treatment (invitation to participate in the Fostering Opportunities program) was assumed to have begun immediately after randomization.

- Students randomized after February 15 were included in the next school year's fall cohort.

Treatment (invitation to participate in the Fostering Opportunities program) was assumed to have begun in August, although some initial outreach to families occurred for some students prior to August.

Description of the Sample

Eligibility for the study was based on being in foster care at the time of enrollment. This means that in most cases, a student who was enrolled in each term was either in foster care at the start of that academic term or in close proximity to the start of that term (e.g., a student entered the study in early August while in foster care and school started late August).

One-Year Outcomes

The sample of youth for whom outcomes were measured at one year after randomization consisted of 230 students who were enrolled in Grades 6-12. These students were all enrolled in Jeffco Public Schools at the time of randomization and had experienced an out-of-home foster care placement. At the time of randomization, most of these young people were in the custody of Jeffco Human Services. Adjacent county human services departments also referred young people into the program if they attended school in Jeffco. There were more Black, Indigenous, and People of Color (BIPOC) youth in the study than non-Hispanic White. See Table 1 for breakdown across year one and year two.

One-hundred and forty-eight (64.3%) students were randomized into the treatment group and were invited to participate in the Fostering Opportunities program. Eighty-two (35.7%) were randomized into the control group (i.e., business as usual). Table 1 presents key demographic characteristics of the sample. The full sample, n=230, was used for Research Questions 1a, 1b, and 1c, which focused on attendance, behavior, and course passing rates within one year of randomization. There were slight variations in sample size per research question due to missing outcome or baseline data.

Two-Year Outcomes

Ninety-seven (60.2%) students had access to the Fostering Opportunities program for at least two years. Sixty-four (39.8%) were randomized into the control group. Table 1 presents key demographic characteristics of the sample. The full sample, n=161, was used for Research Questions 1a, 1b, and 1c, which focused on attendance, behavior, and course passing rates within two years of randomization, with slight variations in sample size per research question due to missing outcome or baseline data.

Table 1. Key Demographic Characteristics of Sample

Sample Size	One-Year Outcomes Sample (n=230)	Two-Year Outcomes Sample (n=161)
Gender	50.0% Female 50.0% Male	47.8% Female 52.2% Male
Average age at first removal	10.5 years old (with a range of 0-18)*	10.3 years old (with a range of 0-18)*
Average age at enrollment	13.4 years old (with a range of 10-18)	13.3 years old (with a range of 10-18)
County of custody	75.2% Jeffco 24.8 % Other counties**	75.8% Jeffco 24.2 % Other counties**
Primary ethnicity	45.2 % Non-Hispanic White	44.1 % Non-Hispanic White

Sample Size	One-Year Outcomes Sample (n=230)	Two-Year Outcomes Sample (n=161)
	54.8 % BIPOC	55.9 % BIPOC
Grade at Enrollment	29.6 % in Grade 6 15.2 % in Grade 7 12.2 % in Grade 8 18.3 % in Grade 9 12.6 % in Grade 10 10.0 % in Grade 11 2.2 % in Grade 12	34.2 % in Grade 6 15.5 % in Grade 7 8.7 % in Grade 8 18.0 % in Grade 9 11.8 % in Grade 10 10.6 % in Grade 11 1.2 % in Grade 12
Students with Special Education designation	27.0 %	26.1 %

Note. *Previous report indicated an enrollment age rate of up to 19. The local child welfare agency reviewed date of birth for accuracy and made some corrections. ** Other counties include Adams, Arapahoe, Denver, Douglas, and Park.

On Track to Graduate

An overlapping sample (n=95) was used to assess aspects of on track to graduate. The criteria for inclusion in this sample was being enrolled in high school during the study period. Sixty-eight students had at least four academic periods of high school enrollment and credit accumulation data during the two-year outcome window. Outcomes for on track to graduation were assessed for a given student only for academic periods in which they were in high school and thus capable of accumulating credits. For one-year outcomes, a student must have had two full academic periods in high school, so a student in eighth grade who was randomized in the spring would not be included in the sample for this outcome because they only had one academic period of high school data during the one-year outcome period.

Missing Data

For each research question, listwise deletion was used to reduce the sample size to the number of students who had complete data. Thus, there are variations in sample size by research question.

Attrition

Attrition refers to how much of the sample was not included in the analysis because there was missing data. In this study, all the missingness was because school records could not be located for a student because they transferred out of Jeffco Public Schools and their current school district was not responsive to requests made by Jeffco Public Schools to receive their transcripts. The What Works Clearinghouse sets the standard for how much attrition can occur in a study before it becomes a threat to the validity, and each contrast or research question is categorized as having *low* or *high* attrition.

Attrition for this study was at the individual level, and the cautious boundary was used for all contrasts, regardless of whether attrition could be reasonably linked to each outcome measure. For each contrast, we calculated overall attrition, the number of individuals missing outcome data divided by the total number of individuals randomized for whom outcome data was possible, as well as differential attrition, which is the absolute value of the difference in attrition by treatment group. Overall attrition for two-year

outcomes was greater, as is to be expected. Regardless, all contrasts in the study were determined to be low attrition. Complete information on attrition rates can be found in Appendix A.

Table 2. Attrition by Contrast

Contrast	High / Low Attrition
One Year Post-Randomization	
Attendance Rate	Low
Course Pass Rate	Low
Suspension Incidents (Likelihood and Count)	Low
On Track to Graduation Rate	Low
Two Years Post-Randomization	
Attendance Rate	Low
Course Pass Rate	Low
Suspension Incidents (Likelihood and Count)	Low
On Track to Graduation Rate	Low

Outcome Measures

Attendance

Attendance was defined as yes/no (present or absent) for the day, regardless of reason or excused/ unexcused status. The attendance measure mirrors Jeffco Public Schools business rules for average daily attendance rates. If a student dropped out of school, it was not possible to discern when that occurred in the semester, so zero days attended were recorded and the number of days used in the denominator was derived from the last academic period for which the student had attendance data.

Course Passing

Course passing was defined as a student receiving a letter grade of "D" or higher on a traditional A-F grading scale or a number grade of 2 or higher on a 1-4 grading scale. A student was considered as failing a course with a number grade of 1 or a letter grade of "F" (failing), "U" (unmet), "NM" (not met), "N" (not passing), "ND" (student does not demonstrate attribute), "NC" (no credit), "RL" (remote learning, assigned in place of an "F" during remote learning), "WP"/"WF" (withdraw), or "IN"/"I" (incomplete). These course pass/fail distinctions were developed in consultation with Jeffco Public Schools.

A course pass rate was calculated for each student for each semester post-randomization and before the intervention. Course pass rates were calculated by summing all courses passed and dividing by all courses taken during the academic periods within one year of randomization. If a student dropped out during a given academic period, the number of courses taken during the last period of available data was used in the denominator (courses taken) and zero courses were considered to be "passed" during the dropout semester.

Suspension Incidents

The percentage of students suspended at least once was defined categorically as whether a student had one or more suspensions and is inclusive of in-school and out-of-school suspensions. The decision to focus on suspension incidents and not differentiate between type of suspension (in-school vs. out-of-school) or number of days is because school-based practices and the use of restorative justice approaches can

influence the type of suspension and length of time students are suspended. If a student dropped out in an academic period, they received zero suspension incidents for that period.

On Track to Graduate

The percentage of students on track to graduate was based on a categorical variable at the student level. Students were considered "on track" for each term of high school coursework in which they earned at least 2.875 credits. This was cumulative for each relevant term within the outcome period of interest. For example, a student with two semesters of high school coursework within the one-year post-randomization period would need to have earned at least 5.75 credits over the course of the two semesters to be considered on track to graduate. It did not matter if this student earned fewer than 2.875 credits in one of the semesters and more in the other semester, provided they earned at least 5.75 credits within one-year post-randomization. Students who dropped out of school were considered to not be on track to graduate, regardless of their credit accumulation.

Analytic Approach

Descriptive Analyses

Pre-post Test Comparisons: Attendance, Suspensions, and Course Pass Rates

The differences between the intervention and control groups baseline measure compared to the baseline to one and two years after starting the study were assessed. This pre-post test descriptive approach illustrates the change over time for the treatment group relative to the control group. This pre-post comparison was made when the treatment and control groups were not equivalent at baseline on the outcome of interest. The rates reported in the results section are an unweighted mean.

Percent Improvement: On Track to Graduate

On track to graduate has a proxy for baseline data of course pass rate. It was not practical to determine if students were on track to graduate at entry into the study as it would have eliminated the students who were in ninth grade at entry to the study because they would have been in eighth grade the year prior. Thus, for this outcome, the percent increase formula was used to determine the descriptive magnitude of improvement. This is not ideal because the treatment and control group differed at baseline on the proxy measure of course pass rate. Students in the control group had a higher rate of passing classes on average than the treatment group, which suggests that the percent improvement could be greater than what is observed.

$$\text{Percent Increase} = \frac{\text{Treatment Value- Control Value}}{\text{Control Value}} \times 100$$

Baseline Equivalence of Demographic Measure

We used administrative data from child welfare to assess baseline equivalence of students between treatment and control groups on demographic variables and foster care placement in the year prior to randomization. Education data from local education agencies were used to examine baseline equivalence on the outcome areas of interest: attendance, course pass rate, and suspensions. Baseline equivalence was assessed by research questions to ensure that appropriate adjustments were made based on the sample of students included in each statistical analysis. For each research question, the outcome of interest was also assessed for baseline equivalency—course pass rate was used for the on track to graduate.

Baseline data were compared using the Hedge's g for continuous variables and the Cox index for dichotomous variables. Absolute values of effect sizes of less than 0.05 were determined to be "equivalent," while values greater than 0.05 and less than 0.25 were in the "adjustment range." All variables were either equivalent or within the adjustment range. Each variable shown to be within the adjustment range was included as a covariate in the statistical models. Tables detailing the baseline equivalence by research question can be found in Appendix B.

Statistical Significance Testing

For all research questions, a priori threshold for statistical significance was set at alpha = 0.05, meaning there was a 95% chance that any differences detected were attributable to the Fostering Opportunities intervention and not random chance. This threshold was selected because we anticipated the overall sample size would be relatively small. *P*-values are reported in the analysis of covariance (ANCOVA) tables.

Covariates

Some of the covariates used in statistical models were finalized prior to the researchers accessing outcome data for the students enrolled in the study. Data from implementation of Fostering Opportunities in Jeffco prior to launch of the RCT were used to determine which covariates should be included. The predetermined threshold for inclusion of an individual covariate was set at explaining 10% of the variance in the outcome of interest or to address baseline equivalence. Grade level explained more than 10% variance in the outcomes of attendance (12%), course pass rate (25%), and suspension (15%). Although age is recommended by the Title IV-E Clearinghouse to be assessed at baseline (if available), grade level was used as a proxy in that it has more meaningful interpretability for an educational intervention. Age at first entry into foster care and foster care placement type explained less than 10% of the variance in all outcome areas of interest, so were not included in the model. Grade level at enrollment, which was included in each statistical model, served as a proxy for age for study participants. Other covariates were included because outcomes of interest and other variables were not equivalent at baseline. Additionally, the project implementation team in consultation with the research team decided to include the cohort or the semester that students were enrolled in the study as a covariate to account for COVID-19 related effects on the outcomes.

Attendance and Course Pass Rate

An outcome of interest was calculated for each student both at baseline and one- and two-years post-randomization. We used a linear ANCOVA model. The primary regressor of interest was an indicator of participation in the intervention ("group"). Other covariates included grade level at randomization, an indicator of the semester of study start ("cohort"), race/ethnicity, gender, special education status, and baseline data on the outcome of interest.

Suspension Incidents

We used a mixed hurdle Poisson regression model, including a normal random effect to account for similarities among known siblings in the study. The primary regressor of interest was an indicator of participation in the intervention ("group"). Other covariates included school level at randomization (i.e., middle or high school), an indicator of the semester of study start ("cohort"), race/ethnicity, gender, special education status, and baseline data on number of suspension incidents.

On Track to Graduate

Because the outcome of interest for this research question was binary (where a "1" indicates that a student is on track to graduate and a "0" indicates that they are not on track to graduate), we leveraged a logistic regression model to estimate treatment and control effects on the outcome variable. The primary regressor of interest was an indicator of participation in the intervention ("group"). Other covariates included grade level at randomization, an indicator of the semester of study start ("cohort"), race/ethnicity, gender, a binary indicator for a foster care placement in the prior year, and baseline data on the outcome of interest. In addition to these covariates, special education status was included in the two-year outcomes model, as baseline equivalence for this contrast determined that the groups were in the adjustment range for this variable.

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Results

Results

For each research question, the results of the statistical models is presented first, followed by descriptive information that provides insight into the magnitude of change or how much rates changed from baseline for the group of students that had access to Fostering Opportunities compared to those that did not.

Understanding the Statistical Models

"Statistical significance" is the determination that access to the Fostering Opportunities program impacted the outcome.

For the purposes of this pilot study, statistical significance was defined as a 95% or greater chance that the difference in outcomes between the treatment and control groups is attributable to the Fostering Opportunities intervention, and not random chance.

- "P-values" of less than 0.05 were predetermined to be statistically significant.
- "Effects" with a positive sign indicate that the outcome of interest went up, whereas negative signs indicate that the outcome of interest went down. This is important because for some outcomes, a positive change is an increase (e.g., attendance), whereas for others, a decrease is an improvement (e.g., number of times students were suspended). The value of the effect is also an indicator of magnitude or strength of the predictor.

Group is the primary variable of interest for determining the impact of Fostering Opportunities. The treatment group is those students who were randomly assigned to have access to the program.

All Variables Except Group indicate the impact of that predictor for all students in the study (i.e., it is not specific to those who had access to Fostering Opportunities.

Baseline refers to the rate of attendance, number of suspensions, or percentage of courses passed in the semester before students were randomized into the Fostering Opportunities treatment or control group.

Cohort refers to the academic term students were enrolled in the study. Cohort was included in the model to provide insight into potential pandemic-related effects. Students who enrolled in the study in spring of 2019 would have a full year of data that was not affected by the pandemic—spring 2019 and fall 2019; this is why that cohort is set as the reference term and comparisons are made against that cohort.

Grade refers to the grade level of the students when they were enrolled in the study and for most students in the study, indicates that they were in foster care at the start of that academic term. Grade level was included in the model to provide insight into whether the outcomes of interest, on average, differ by grade levels. This is intended to inform future implementation of educational interventions for youth in foster care.

Research Questions

Research Questions:

1. **What is the impact of Fostering Opportunities on students'**
   - f. **attendance rate,**
   - g. **course pass rate,**
   - h. **odds of being suspended,**
   - i. **number of times being suspended among those students who were suspended at least once, and**
   - j. **being on track for high school graduation**

at one and two years after randomization?

What Does "At One Year and Two Years After Randomization" Really Mean?

Students were randomized into the treatment (i.e., intervention) or control group (i.e., business as usual) to determine if each student would have access to Fostering Opportunities or not. A random number generator was used to ensure each student had an equal chance of accessing Fostering Opportunities. Randomization occurred at the start of each academic term (i.e., fall or spring semester).

One Year Later outcomes were measured by counting all the data in that time period. For example, attendance rate was based on the percent of days attended for two semesters. For students randomized in the fall, that would include the full academic year (i.e., fall and spring). For students randomized in the spring, it would include their attendance that spring semester and the following fall semester.

Two Years Later outcomes were measured by counting four semesters of attendance, suspension incidents, grades (course pass rate), and credit accumulation (on track to graduate).

We interpret the findings of the differences occurring "within one year" or "within two years." If outcomes were significantly different at the year one and year two marks, then we say the improvement was "sustained" because there was evidence of attendance among students who had access to Fostering Opportunities. For example, outcomes improved within the first year and was still better than the control group when we look at two full years of data.

Research Question 1A: Attendance

Statistical Model of Attendance Rate

The Fostering Opportunities program improved attendance rates within one year and were sustained at the two-year follow-up.

One Year Later. For the ANCOVA regression model with attendance rate one year after randomization into the study, results are shown in Table 3. The Fostering Opportunities program led to improved attendance rates ($p = 0.02$). The positive sign on the effect is evidence that the attendance rates in the treatment group were higher than the control group. The model also indicates that baseline attendance rates for all students are the strongest predictor of attendance rates one year later ($p = 0.00$). Relative to students

who enrolled in the study prior to the COVID-19 pandemic, all students in the spring 2022 and fall 2022 cohorts had lower attendance rates on average (i.e., this is not associated with the treatment).

Two Years Later. Results of the ANCOVA regression model with attendance rate two years after randomization into the study are shown in Table 4. The Fostering Opportunities program led to statistically significant differences in attendance rates at the two-year mark (p = 0.01), with students with access to the program attending more days of school, on average. As with the one-year outcomes model, baseline attendance for all students was a strong indicator of attendance rates two years later (p = 0.00). This model also indicated racial and ethnic disparities in attendance rates for all students, with non-Hispanic White students attending at marginally higher rates than their BIPOC peers (p = 0.04). There were not enough students in this study to examine the interactions between Fostering Opportunities and race to determine if the intervention works differently for BIPOC versus non-Hispanic White students. All students in the fall 2020 cohort had lower attendance than their peers in the spring 2019 cohort (p= 0.02) (i.e., this is not associated with the intervention).

Table 3. Results of Attendance Rate ANCOVA Regression Model at One Year Later (Post-Randomization)

Variable	Effect	Standard Error	p-Value
Group			
Treatment	0.07	0.03	0.02*
Control (Ref)	--	--	--
Baseline			
Baseline Attendance Rate	0.31	0.06	0.00*
Race & Ethnicity			
Non-Hispanic White	0.02	0.03	0.39
BIPOC (Ref)	--	--	--
Gender			
Female	-0.03	0.03	0.20
Male (Ref)	--	--	--
Special Education Ever			
Yes	0.05	0.03	0.10
No (Ref)	--	--	--
Cohort			
Spring 2019 (Ref)	--	--	--
Fall 2019	-0.04	0.05	0.40
Spring 2020	-0.03	0.05	0.53
Fall 2020	-0.08	0.05	0.11
Spring 2021	-0.02	0.04	0.63
Fall 2021	-0.06	0.05	0.24
Spring 2022	-0.17	0.07	0.02*
Fall 2022	-0.13	0.05	0.01*
Grade			
Grade 6 (Ref)	--	--	--
Grade 7	-0.04	0.04	0.34
Grade 8	-0.01	0.05	0.79
Grade 9	0.00	0.04	0.92
Grade 10	-0.04	0.04	0.34

Variable	Effect	Standard Error	p-Value
Grade 11	-0.03	0.05	0.58
Grade 12	0.04	0.09	0.66

Notes. * p = < 0.05.
Ref refers to the reference group, the baseline group against which the remaining groups are evaluated. For instance, in the model above, the effects reported for each grade level are the differences between that grade level and the students that enrolled in Grade 6, holding all other variables constant.

Table 4. Results of Attendance Rate ANCOVA Regression Model at Two Years Later (Post-Randomization)

Variable	Effect	Standard Error	p-Value
Group			
Treatment	0.09	0.03	0.01*
Control (Ref)	--	--	--
Baseline			
Baseline Attendance Rate	0.33	0.07	0.00*
Race & Ethnicity			
Non-Hispanic White	0.07	0.03	0.04*
BIPOC (Ref)	--	--	--
Gender			
Female	0.03	0.03	0.41
Male (Ref)	--	--	--
Special Education Ever			
Yes	-0.02	0.03	0.55
No (Ref)	--	--	--
Cohort			
Spring 2019 (Ref)	--	--	--
Fall 2019	-0.11	0.05	0.05
Spring 2020	-0.05	0.05	0.37
Fall 2020	-0.13	0.05	0.02*
Spring 2021	-0.03	0.05	0.55
Fall 2021	-0.02	0.06	0.68
Grade			
Grade 6 (Ref)	--	--	--
Grade 7	-0.07	0.05	0.16
Grade 8	0.00	0.06	0.99
Grade 9	0.01	0.05	0.84
Grade 10	0.01	0.05	0.91
Grade 11	-0.07	0.06	0.20

Note. Ref refers to the reference group, the baseline group against which the remaining groups are evaluated. For instance, in the model above, the effects reported for each grade level are the differences between that grade level and the students that enrolled in Grade 6, holding all other variables constant.

Descriptive Analysis

One Year Later

- **Students who had access to the Fostering Opportunities program had a 6.44 percentage point improvement in their average daily attendance rate one year later.**
- **While students in the control group had a 1.47 percentage point decrease in their average daily attendance rate one year later.**

Two Years Later

- **Students with access to Fostering Opportunities had a 5.40 percentage point improvement in average daily attendance two years later.**
- **While the students in the control group had a 5.35 percentage point decrease in their average daily attendance rate two years later.**

The change in attendance rate from baseline is illustrated in Figures 1 and 2. The consistent trend is that attendance rates improved over time for students who had access to Fostering Opportunities, while attendance rates declined for those in the control group. The attendance rates that are inclusive of two full years are particularly noteworthy—attendance rates for students who had access to Fostering Opportunities increased by approximately the same amount as the attendance rates decreased for the control group. This suggests that not only does Fostering Opportunities improve students' daily attendance rates, but also in the absence of intervention, students who experience foster care might have a drop in attendance.

Why Are Baseline Rates Different for One- and Two-Year Outcomes?

The two-year rates are a sub-sample of the students included in the one-year rates, meaning that not all students in the study had two years of data. They may have enrolled later in the project, or they moved to a different school district and their transcripts were not provided when requested for this study. This is why the sample size (*n*) is smaller for the two-years-later results and the baseline rates are not exactly the same.

Figure 1. Mean Attendance Rate by Group, at Baseline and One Year Later (Post-Randomization)

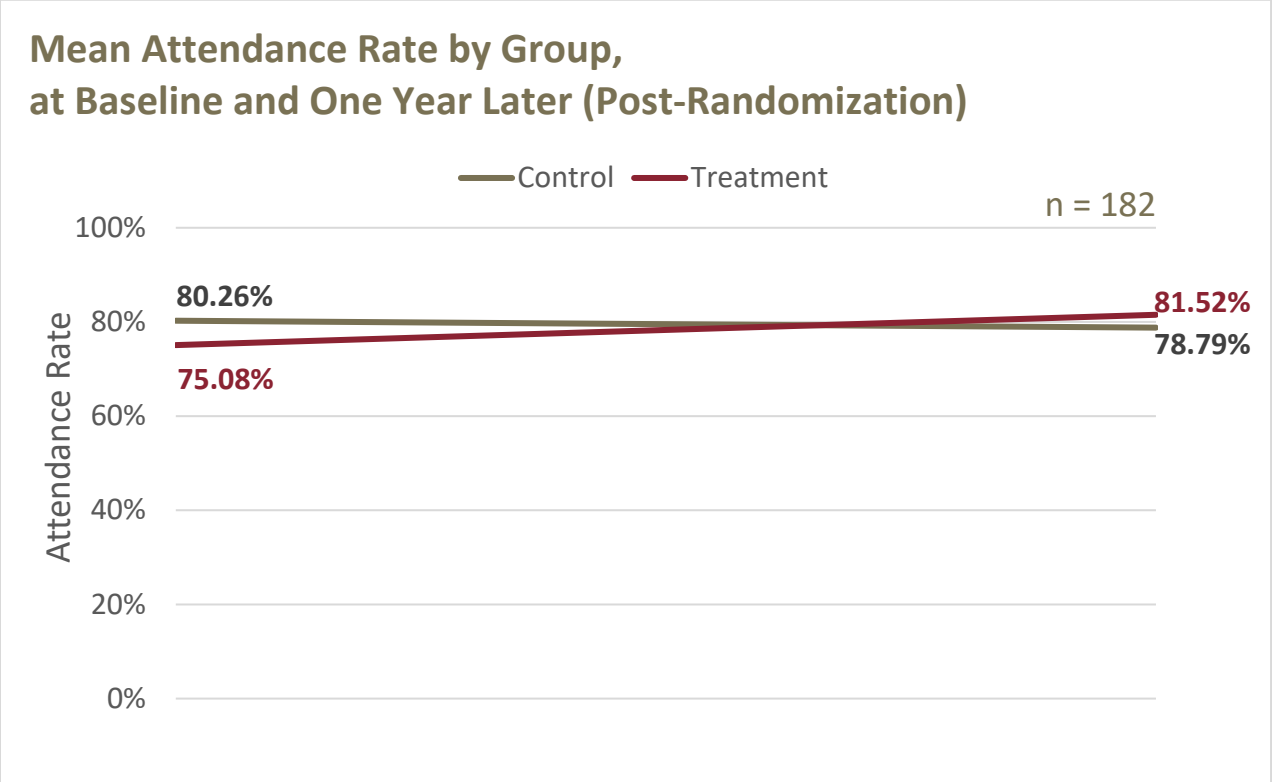
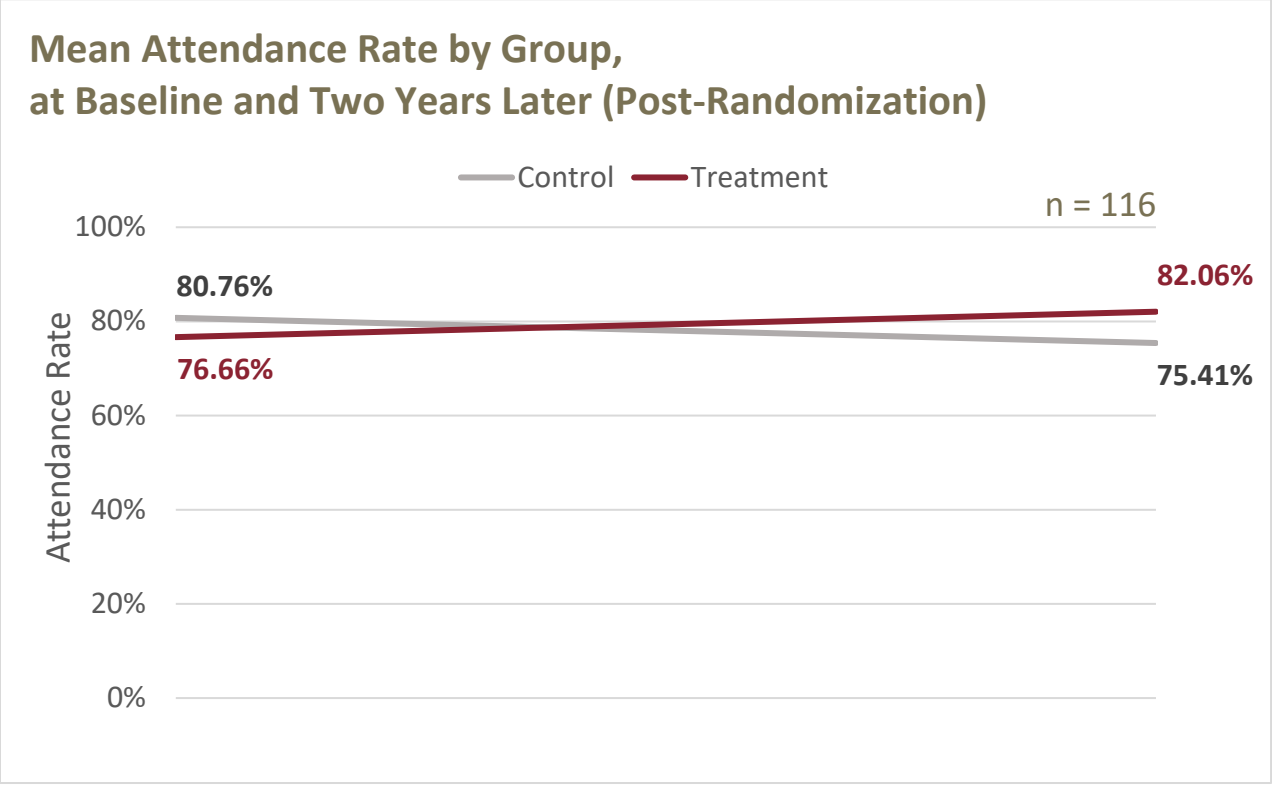


Figure 2. Mean Attendance Rate by Group, at Baseline Two Years Later (Post-Randomization)



Research Question 1B: Course Passing

Statistical Model of Course Pass Rate

The Fostering Opportunities program did not impact course pass rates within one year, but by the end of two years students who had access to fostering opportunities were passing more classes on average than the control group.

One Year Later. For the ANCOVA regression model with course pass rate one year after randomization into the study, results are shown in Table 5. There was no statistically significant difference between the treatment and control group, meaning that the program did not impact course pass rate within one year of randomization into the study. It also means there was no evidence of harm due to the implementation of this program. In this model, only baseline course passing rates influenced course passing rates one year after randomization with statistical significance (p = 0.00), meaning students past academic performance was predictive of their future academic performance, regardless of whether they had access to the intervention.

Two Years Later. Results of the ANCOVA regression model with course pass rate two years after randomization into the study are presented in Table 6. There was a statistically significant difference between the treatment and control groups, with students in the treatment group passing more courses on average than their peers in the control group (p = 0.02). As described in the Methods section, this improvement is inclusive of all courses the student enrolled in over the two-year period. As with the previous model, baseline course passing rates are a significant indicator of course passing rates for all students two years later (p = 0.00). Additionally, non-Hispanic White students had higher course pass rates at this point than their BIPOC peers (p = 0.01), regardless of treatment status—there were not enough students enrolled in the study to examine the interaction effects of race, ethnicity, and the program. This two-year outcome is inclusive of the courses that were passed during both year one and year two.

Table 5. Results of Course Pass Rate ANCOVA Regression Model at One Year Later (Post-Randomization)

Variable	Effect	Standard Error	p-Value
Group			
Treatment	0.04	0.05	0.39
Control (Ref)	--	--	--
Baseline			
Baseline Course Pass Rate	0.44	0.06	0.00*
Race & Ethnicity			
Non-Hispanic White	0.06	0.04	0.19
BIPOC (Ref)	--	--	--
Gender			
Female	-0.02	0.04	0.69
Male (Ref)	--	--	--
Special Education Ever			
Yes	0.04	0.04	0.39
No (Ref)	--	--	--
Cohort			
Spring 2019 (Ref)	--	--	--
Fall 2019	-0.04	0.13	0.76
Spring 2020	0.01	0.13	0.96

Variable	Effect	Standard Error	p-Value
Fall 2020	-0.02	0.14	0.88
Spring 2021	0.02	0.13	0.89
Fall 2021	0.03	0.14	0.85
Spring 2022	0.04	0.16	0.79
Fall 2022	0.05	0.14	0.69
Grade			
Grade 6 (Ref)	--	--	--
Grade 7	-0.09	0.07	0.19
Grade 8	-0.04	0.07	0.51
Grade 9	-0.07	0.06	0.28
Grade 10	-0.04	0.07	0.59
Grade 11	-0.05	0.08	0.53
Grade 12	0.10	0.12	0.43

Note. Ref refers to the reference group, the baseline group against which the remaining groups are evaluated. For instance, in the model above, the effects reported for each grade level are the differences between that grade level and the students that enrolled in Grade 6, holding all other variables constant.

Table 6. Results of Course Pass Rate ANCOVA Regression Model at Two Years Later (Post-Randomization)

Variable	Effect	Standard Error	p-Value
Group			
Treatment	0.11	0.05	0.02*
Control (Ref)	--	--	--
Baseline			
Baseline Attendance Rate	0.46	0.07	0.00*
Race & Ethnicity			
Non-Hispanic White	0.12	0.05	0.01*
BIPOC (Ref)	--	--	--
Gender			
Female	0.08	0.05	0.09
Male (Ref)	--	--	--
Special Education Ever			
Yes	0.04	0.05	0.42
No (Ref)	--	--	--
Cohort			
Spring 2019 (Ref)	--	--	--
Fall 2019	-0.10	0.12	0.39
Spring 2020	0.08	0.12	0.50
Fall 2020	-0.01	0.13	0.93
Spring 2021	0.13	0.12	0.30
Fall 2021	0.08	0.12	0.51
Grade			
Grade 6 (Ref)	--	--	--
Grade 7	-0.14	0.08	0.09
Grade 8	-0.09	0.08	0.28
Grade 9	-0.09	0.07	0.22

Variable	Effect	Standard Error	p-Value
Grade 10	-0.10	0.07	0.17
Grade 11	-0.12	0.08	0.15

Note. Ref refers to the reference group, the baseline group against which the remaining groups are evaluated. For instance, in the model above, the effects reported for each grade level are the differences between that grade level and the students that enrolled in Grade 6, holding all other variables constant.

Descriptive Analysis

One Year Later

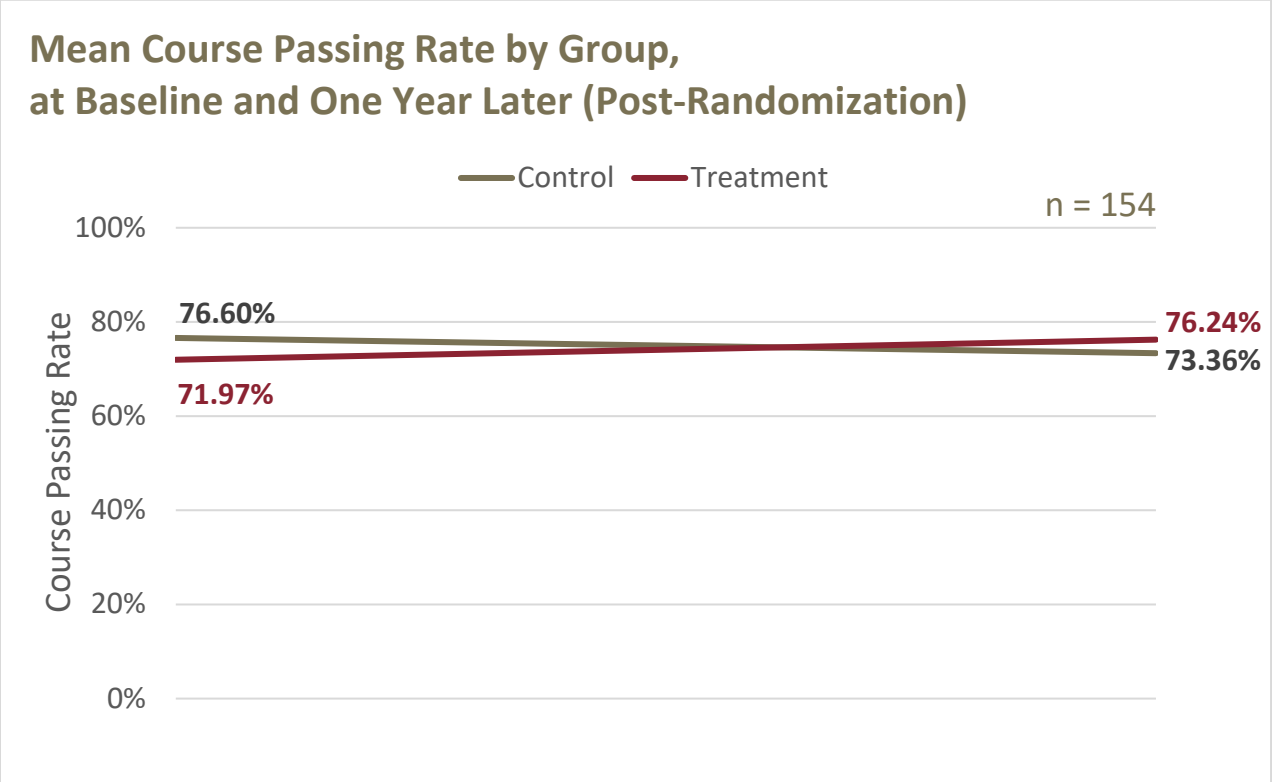
- **Students who had access to the Fostering Opportunities program had a 4.33 percentage point improvement in their course pass rate over the course of one year.**
- **While students in the control group had a 3.24 percentage point decrease in course pass rate over the course of one year.**

Two Years Later

- **Students who had access to the Fostering Opportunities program had a 4.96 percentage point improvement in their courses pass rate over the course of two years.**
- **While students in the control group had an 8.34 percentage point decrease in course pass rate over the course of two years.**

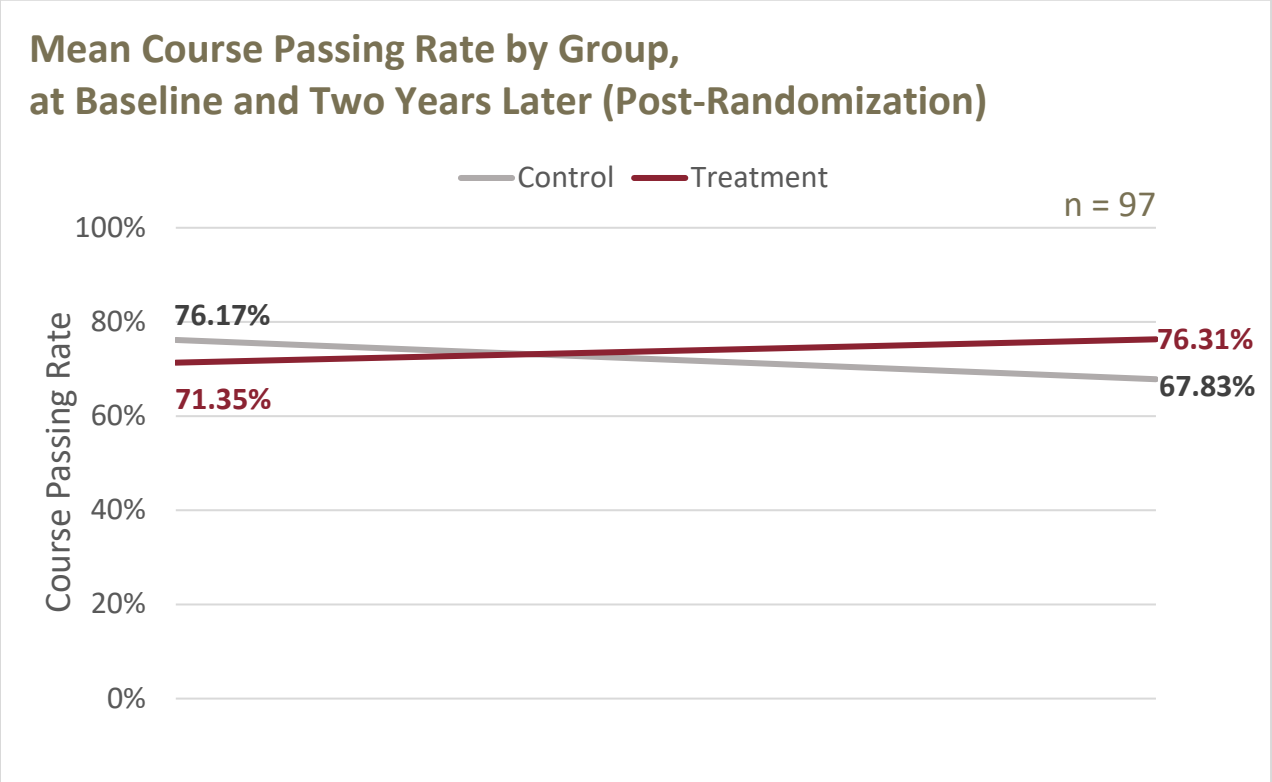
The change in course pass rate from baseline is illustrated in Figures 3 and 4. These analyses suggest that students who have access to Fostering Opportunities *may* begin to pass more courses within the first year, but the academic performance benefit of the program is fully realized in the second year. The practically meaningful decline in course pass rate from baseline for the control group suggests that not intervening may be exacerbating poor educational outcomes for students who experience foster care over time.

Figure 3. Mean Course Passing Rate by Group, at Baseline and One Year Later (Post-Randomization)



Note. These statistical models indicate that the observed difference between the treatment and control group, when controlling for baseline were not significant. This figure provides context for the statistically significant differences found within two years (Figure 4).

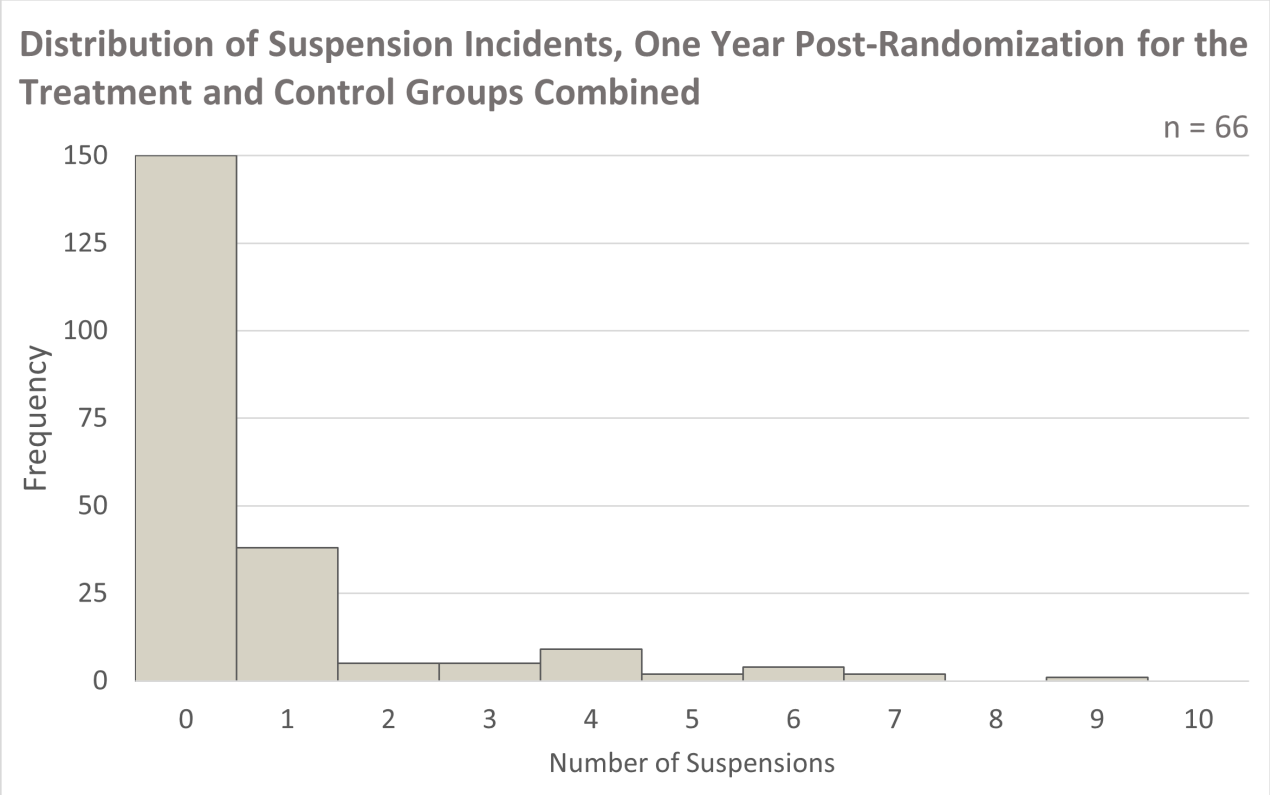
Figure 4. Mean Course Passing Rate by Group, at Baseline and Two Years Later (Post-Randomization)



Research Questions 1C and 1D: Suspension Incidents

For the purposes of this report, suspension incidents are measured in two ways: a) whether a student had any suspensions during the time period of interest (one or two years); and b) for those students who were suspended at least once, the number of times a student is suspended. The majority of students were not suspended at all, as illustrated in Figure 5 which is the distribution of the number of suspension incidents for all students in the study (i.e., not separated by treatment and control groups).

Figure 5. Histogram of Suspension Incidents One Year Later (Post-Randomization)



Statistical Model of Suspensions

For the mixed hurdle Poisson regression model, suspensions as the longitudinal outcome simultaneously models the likelihood of a suspension occurring at all and, for the subset of students who were suspended at least once, the frequency of suspension incidents is also modeled. Results are shown in Tables 7 and 8.

The Fostering Opportunities program did not impact the likelihood of being suspended, but for those students who were suspended at least once, it did decrease the number of times they were suspended.

Likelihood of a Suspension Within One Year. The results of the regression model indicated that access to Fostering Opportunities did not impact the likelihood of suspension. The p-value of 0.06 is approaching significance and the sample size is relatively small for this study, so it is possible that this is a Type II error, failure to detect a significant impact. The number of suspensions a student received at baseline was a statistically significant indicator that a student would be suspended within one year of randomization ($p = 0.02$), regardless of whether they received access to the intervention. Additionally, high school students were more likely to be suspended at least once ($p = 0.00$) than middle school students—this school-level effect for all students in the study.

Likelihood of a Suspension Within Two Years Later. At two years after randomization, students in the treatment group were no more or less likely than students in the control group to have at least one suspension.

Number of Suspension Incidents

Within One Year Later. Fostering Opportunities decreased the number of suspension incidents for students who were suspended at least once, compared to their peers in the control group (p = 0.00). When compared to their non-Hispanic White peers who were suspended, BIPOC students had a higher number of suspension incidents, on average, regardless of treatment status (p = 0.00). All students who were suspended and had a specialized education plan at any point during the study, had more suspension incidents, on average, than their peers who were suspended, but were never identified as eligible for special education services (p = 0.02).

Number of Suspension Incidents Within Two Years Later. Fostering Opportunities led to a statistically significant decrease in the number of suspensions for students who were suspended at least once (p = 0.00). Non-Hispanic White students in both the treatment and control groups had fewer suspension incidents, on average, than their BIPOC peers (p = 0.00). Students who were suspended and had received specialized education services at any point during the study had more suspension incidents, on average, than their peers who were suspended, but never were identified as eligible for special education services (p = 0.03), regardless of treatment status.

Table 7. Results of Suspensions Mixed Hurdle Poisson Regression Model One Year Later (Post-Randomization)

Variable	Effect	Standard Error	p-Value
Model for Likelihood of a Suspension			
Group			
Treatment	-0.86	0.45	0.06
Control (Ref)	--	--	--
Baseline			
Baseline Number of Suspensions	0.76	0.32	0.02*
Race & Ethnicity			
Non-Hispanic White	0.23	0.39	0.55
BIPOC (Ref)	--	--	--
Special Education Ever			
Yes	-0.37	0.41	0.37
No (Ref)	--	--	--
Cohort			
Spring 2019 (Ref)	--	--	--
Fall 2019	-1.34	1.14	0.24
Spring 2020	-0.47	1.13	0.68
Fall 2020	-1.56	1.22	0.20
Spring 2021	-0.86	1.11	0.44
Fall 2021	0.06	1.15	0.96
Spring 2022	0.44	1.33	0.74
Fall 2022	-0.32	1.13	0.78
High School Enrollment			
Middle School (Ref)	--	--	--
High School	-1.42	0.43	0.00*

Variable	Effect	Standard Error	p-Value
Model for Frequency or Number of Suspension Incidents			
Group			
Treatment	-1.20	0.33	0.00*
Control (Ref)	--	--	--
Baseline			
Baseline Number of Suspensions	0.11	0.12	0.38
Race & Ethnicity			
Non-Hispanic White	-1.16	0.34	0.00*
BIPOC (Ref)	--	--	--
Special Education Ever			
Yes	0.63	0.28	0.02*
No (Ref)	--	--	--
Cohort			
Spring 2019 (Ref)	--	--	--
Fall 2019	0.55	0.53	0.30
Spring 2020	-1.43	0.72	0.05
Fall 2020	-11.14	180.36	0.95
Spring 2021	-0.39	0.46	0.39
Fall 2021	0.51	0.55	0.35
Spring 2022	-0.54	0.68	0.43
Fall 2022	1.07	0.61	0.08
High School Enrollment			
Middle School (Ref)	--	--	--
High School	-0.18	0.38	0.64

Note. Ref refers to the reference group, the baseline group against which the remaining groups are evaluated. For instance, in the model above, the effects reported for each grade level are the differences between that grade level and the students that enrolled in Grade 6, holding all other variables constant.

Table 8. Results of Suspensions Mixed Hurdle Poisson Regression Model Two Years Later (Post-Randomization)

Variable	Effect	Standard Error	p-Value
Model for Likelihood of a Suspension			
Group			
Treatment	-0.63	0.52	0.23
Control (Ref)	--	--	--
Baseline			
Baseline Number of Suspensions	0.58	0.43	0.18
Race & Ethnicity			
Non-Hispanic White	-0.44	0.48	0.36
BIPOC (Ref)	--	--	--
Gender			
Female	-0.85	0.50	0.09
Male (Ref)	--	--	--

Variable	Effect	Standard Error	p-Value
Special Education Ever			
Yes	-0.10	0.50	0.84
No (Ref)	--	--	--
Cohort			
Spring 2019 (Ref)	--	--	--
Fall 2019	-0.97	1.47	0.51
Spring 2020	-1.18	1.49	0.43
Fall 2020	0.25	1.48	0.87
Spring 2021	-0.35	1.41	0.80
Fall 2021	-0.07	1.46	0.96
			High School Enrollment
Middle School (Ref)	--	--	--
High School	-1.32	0.50	0.01*
Model for Frequency or Number of Suspension Incidents			
Group			
Treatment	-0.74	0.21	0.00*
Control (Ref)	--	--	--
Baseline			
Baseline Number of Suspensions	0.21	0.18	0.23
Race & Ethnicity			
Non-Hispanic White	-0.74	0.22	0.00*
BIPOC (Ref)	--	--	--
Gender			
Female	-0.02	0.23	0.94
Male (Ref)	--	--	--
Special Education Ever			
Yes	0.47	0.22	0.03*
No (Ref)	--	--	--
Cohort			
Spring 2019 (Ref)	--	--	--
Fall 2019	0.13	0.44	0.77
Spring 2020	0.02	0.52	0.98
Fall 2020	-0.53	0.57	0.35
Spring 2021	0.28	0.39	0.48
Fall 2021	0.72	0.41	0.08
High School Enrollment			
Middle School (Ref)	--	--	--
High School	-0.58	0.30	0.06

Note. Ref refers to the reference group, the baseline group against which the remaining groups are evaluated. For instance, in the model above, the effects reported for each grade level are the differences between that grade level and the students that enrolled in Grade 6, holding all other variables constant.

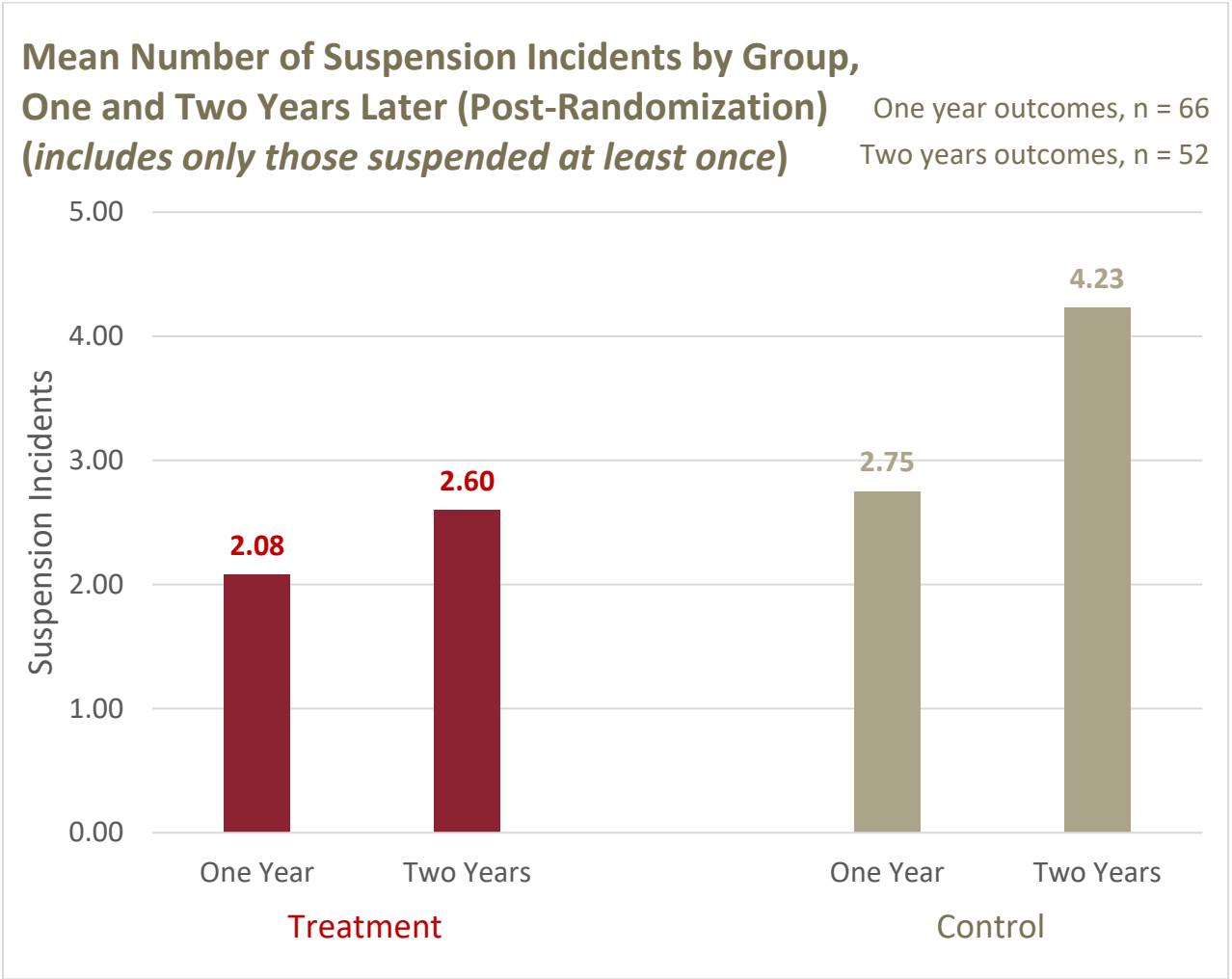
Descriptive Analysis

Of those students who were suspended at least once, students who had access to Fostering Opportunities were suspended 1.63 fewer times over the course of two years.

Percent of Students Suspended. Discussion related to the percent of students suspended is not included because the statistical model indicates that any observed difference in the percentage of students in the treatment versus the control group during the first or second year of the study is most likely attributable to differences in baseline or demographic characteristics.

Number of Suspensions Among Those Students Suspended At Least Once. One year after randomization, students in the treatment group were suspended an average of 2.08 times, compared to 2.75 suspensions on average for the control group. Within two years of randomization, the average number of suspension incidents for those with at least one suspension in the treatment group was 2.60, while students with a suspension in the control group had an average of 4.23 incidents during the same timeframe. These results are presented in a bar chart in Figure 6 (instead of a line graph like attendance and course pass rate) because the statistical model indicated the baseline rate for number of suspension incidents did not significantly predict the outcomes within one or two years.

Figure 6. Mean Number of Suspension Incidents by Group, One and Two Years Later (Post-Randomization).



Research Question 1E: On Track to Graduation

This research question examines on track to graduate through the lens of credit accumulation. Students were deemed "on track" if they had accumulated the expected number of credits relative to their grade level.

This Study Did Not Have Enough Students in High School to Determine if Fostering Opportunities Impacts Graduation Rates

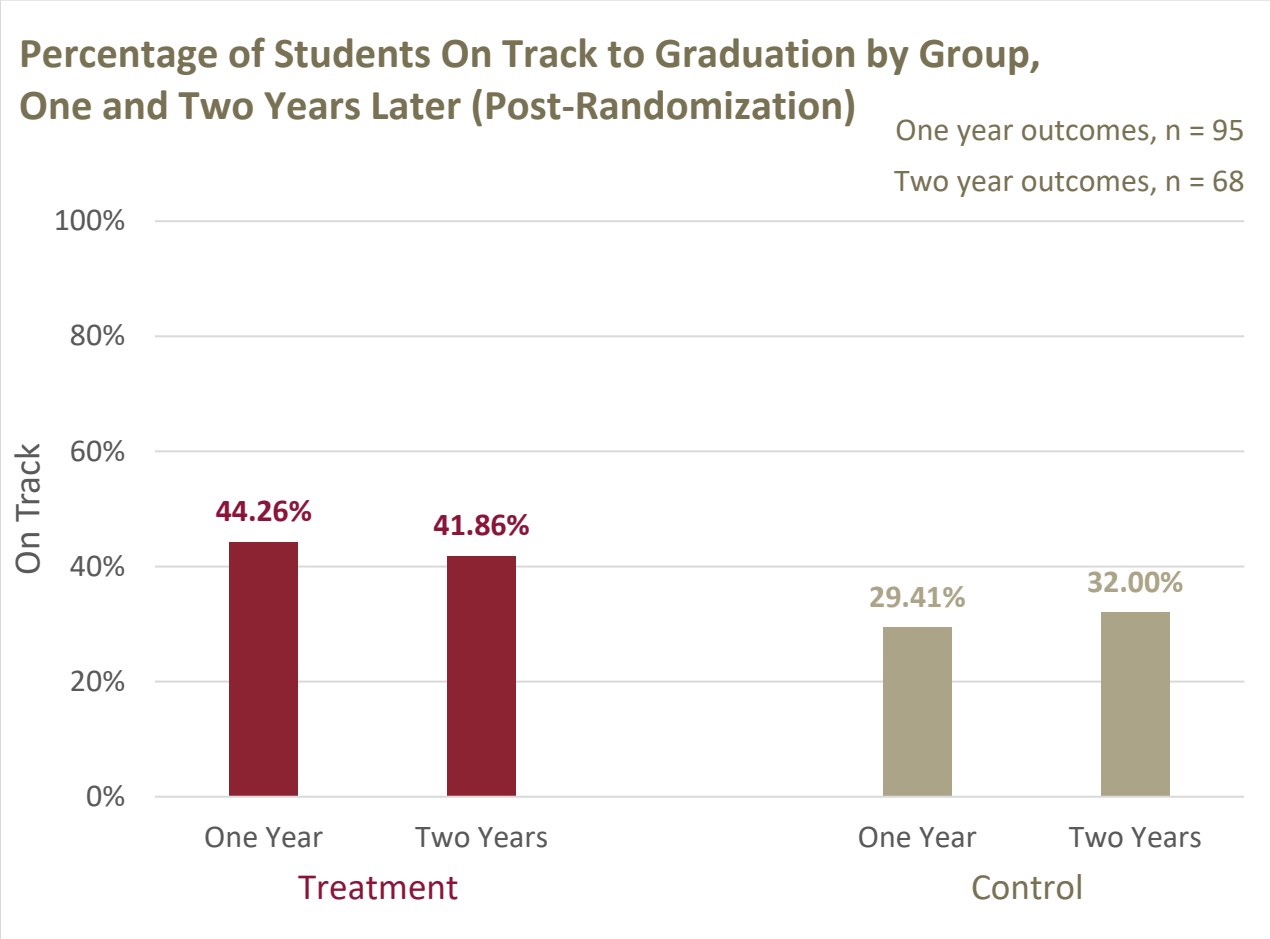
Descriptive results are presented first as observed rates and may be the best available indicator of progress toward closing the high school graduation rate. Results of the statistical model are presented second.

- **One year later, 44.62% of students who had access to Fostering Opportunities were on track to graduate, while 29.41% of students in the control group were on track to graduate.**
- **Two years later, 41.86% of students who had access to Fostering Opportunities were on track to graduate, while 32.00% of students in the control group were on track to graduate.**
- **These observed differences were not statistically significant and that *may* be because the sample size was too small to determine if these observed differences were the result of the intervention or chance.**

One year after access to the Fostering Opportunities program, there was a 14.85 percentage point observed difference between the treatment and control group or a 50.49% increase in the number of students who were on track to graduate. This observed difference, while not statistically significant, is practically meaningful.

Two years after access to Fostering Opportunities, the magnitude of the difference was smaller but still practically meaningful. The lack of statistical significance may be a result of the very small sample size, or it is possible that these observed differences reflect chance. Continuing to evaluate the impact of Fostering Opportunities on getting students on track to graduate is important to understanding the potential return on investment for this intervention.

Figure 7. Percentage of Students on Track to Graduation by Group, One and Two Years Later (Post-Randomization)



Statistical Model of On Track to Graduation Rate

There were not enough students in high school during this study period to determine if Fostering Opportunities impacted their progress toward graduation.

For the logistic regression model, with the on track to graduation rate based on credit accumulation, results are shown in Tables 9 and 10. Access to the Fostering Opportunities program did not demonstrate statistically significant improvements in the percentage of students who were on track to graduate, nor did it decrease students' likelihood of being on track. The only covariates in the model that contributed to a student's likelihood of being on track to graduate was their course passing rate at baseline ($p = 0.04$ at one year, $p = 0.00$ at two years post-randomization) and, for two-year outcomes, female students were more likely than their male counterparts to pass their courses ($p = 0.03$).

The sample size or number of students in high school for at least one full year during the study was too small to determine if Fostering Opportunities impacts high school graduation. There were only 66 students with both baseline and outcome data when on track to graduate was assessed one year later and only 36 students when on track to graduate was assessed two years later.

Table 9. Results of On Track to Graduation Rate Logistic Regression Model at One Year Later (Post-Randomization)

Variable	Effect	Standard Error	p-Value
Group			
Treatment	0.14	0.43	0.74
Control (Ref)	--	--	--
Baseline			
Baseline Course Pass Rate	1.09	0.52	0.04*
Race & Ethnicity			
Non-Hispanic White	0.20	0.39	0.61
BIPOC (Ref)	--	--	--
Gender			
Female	0.50	0.39	0.20
Male (Ref)	--	--	--
Prior Year Removal			
Yes	-0.10	0.40	0.79
No (Ref)	--	--	--
Cohort			
Fall 2019 (Ref)	--	--	--
Spring 2020	0.15	0.74	0.84
Fall 2020	0.55	0.77	0.47
Spring 2021	0.08	0.76	0.92
Fall 2021	0.63	0.79	0.43
Spring 2022	0.48	0.97	0.62
Fall 2022	-0.06	0.86	0.95
Grade			
Grade 9 (Ref)	--	--	--
Grade 10	-0.18	0.41	0.66
Grade 11	-0.22	0.52	0.67
Grade 12	0.16	0.80	0.84

Notes. There were no students in this contrast from the spring 2019 cohort with baseline data on course pass rates, one of the controls included in the logistic regression model. For this reason, the fall 2019 cohort serves as the reference group for cohort effects.
Ref refers to the reference group, the baseline group against which the remaining groups are evaluated. For instance, in the model above, the effects reported for each grade level are the differences between that grade level and the students that enrolled in Grade 6, holding all other variables constant.

Table 10. Results of On Track to Graduation Rate Logistic Regression Model at Two Years Later (Post-Randomization)

Variable	Effect	Standard Error	p-Value
Group			
Treatment	0.44	0.57	0.44
Control (Ref)	--	--	--
Baseline			
Baseline Course Pass Rate	2.88	0.88	0.00*
Race & Ethnicity			
Non-Hispanic White	-0.54	0.50	0.28
BIPOC (Ref)	--	--	--
Gender			
Female	1.01	0.48	0.03*
Male (Ref)	--	--	--
Prior Year Removal			
Yes	0.13	0.54	0.81
No (Ref)	--	--	--
Special Education Ever			
Yes	0.19	0.49	0.70
No (Ref)	--	--	--
Cohort			
Fall 2019 (Ref)	--	--	--
Spring 2020	0.53	0.86	0.54
Fall 2020	0.64	0.80	0.42
Spring 2021	0.37	0.83	0.66
Fall 2021	0.24	0.88	0.79
Grade			
Grade 9 (Ref)	--	--	--
Grade 10	-0.21	0.50	0.68
Grade 11	0.04	0.70	0.95

Note: There were no students in this contrast from the spring 2019 cohort with baseline data on course pass rates, one of the controls included in the logistic regression model. for this reason, the fall 2019 cohort serves as the reference group for cohort effects.
Ref refers to the reference group, the baseline group against which the remaining groups are evaluated. For instance, in the model above, the effects reported for each grade level are the differences between that grade level and the students that enrolled in Grade 6, holding all other variables constant.

Limitations

1. **The COVID-19 pandemic** necessitated remote and hybrid learning during the study timeframe. Jeffco Public Schools staff members shared examples of how the pandemic affected the outcomes of interest. For example:

   - **Attendance:** During spring 2020, when schools quickly transitioned to remote learning, the way attendance was measured varied among schools. Some schools stopped taking attendance. By fall 2020, there was more consistency in collecting attendance data but transitions in and out of remote learning and hybrid delivery continued to affect measurement of this outcome.
   - **Course Pass Rate:** During spring 2020, the district implemented a policy that grades could only improve after the transition to remote learning, but no student's grade would be lowered after that point in time. This likely contributed to extremely high course pass rates for both groups during the spring 2020 time period.
   - **Suspension Incidents:** Throughout the pandemic, there were very few suspension incidents districtwide. Students were primarily not physically in school buildings. Zero students in the study were suspended during the fall 2020 semester.

   Thus, a control was added to the statistical models for the term that students entered the study. Throughout the report, this context provided when findings were statistically significant.

2. **The sample size** is relatively small for an RCT. There were simply fewer youth who met the study criteria of being in foster care and enrolled in Jeffco Public Schools than expected. This is a limitation because it means that the statistical analyses were underpowered, and it is possible that the intervention was effective in some areas that were deemed "insignificant." In statistics, this is called a Type II error, or a false negative result. Thus, the Colorado Lab recommends ongoing research to assess the impact of the intervention on high school graduation as descriptively the "on track to graduate" analysis suggest promise, but statistically the findings were not significant, and the sample size was small.

3. **One geographic area** was the site for the pilot study. Child welfare and education practices and collaboration likely affected outcomes. The strong partnerships and communication among child welfare and education leaders helped ensure the program was delivered with fidelity. Restorative justice practices in schools is an example of an education policy that likely systematically reduced the number of suspensions for students in both the treatment and control groups. Thus, when this program is implemented in other geographic areas, it is important to monitor fidelity and track program outcomes to ensure the program is working as it is intended and actively support continuous quality improvement.

Appendix A: Attrition

Table A-1. Overall and Differential Attrition by Contrast

Contrast	Overall Attrition	Treatment	Control	Differential Attrition	High or Low Attrition
One Year Later (Post-Randomization)					
Attendance Rate	24, 9.92%	15, 9.6%	9, 10.6%	1.03	Low
Course Pass Rate	25, 10.33%	15, 9.6%	10, 11.8%	2.21	Low
Suspension Incidents (Likelihood and Count)	26, 10.74%	17, 10.8%	9, 10.6%	0.24	Low
On Track to Graduation Rate	13, 12.04%	8, 11.6%	5, 12.8%	1.23	Low
Two Years Later (Post-Randomization)					
Attendance Rate	57, 29.69%	35, 30.4%	22, 28.6%	1.86	Low
Course Pass Rate	52, 27.08%	31, 27.0%	21, 27.3%	0.32	Low
Suspension Incidents (Likelihood and Count)	58, 30.21%	36, 31.3%	22, 28.6%	2.73	Low
On Track to Graduation Rate	26, 25.00%	15, 23.4%	11, 27.5%	4.06	Low

Note. Overall attrition is a percentage, whereas *differential attrition* is the percentage point difference in attrition between the treatment and control group (absolute value).

Appendix B: Baseline Equivalency Results

Table B-1. Baseline Equivalence Results for Research Question 1a: Attendance Rate at One Year Later (Post-Randomization) (n=218). (Variables in bold were included in the statistical models because their effect sizes were within the "adjustment range." Variables not in bold were equivalent at baseline.)

	Treatment	Control	Missing	Pooled Std Deviation	Hedges g	Cox's d
Child Welfare Data						
Gender (% Female)	48.59%	51.32%	0.00%			0.06
Race/Ethnicity (% Non-Hispanic White)	43.66%	48.68%	0.00%			0.12
Foster Care Placement Prior Year (Yes)	61.97%	60.53%	0.00%			0.04
Education Data						
Special Education Ever (Yes)	30.28%	23.68%	0.00%			0.20
Attendance	75.08%	80.26%	16.51%	0.24	0.22	

Table B-2. Baseline Equivalence Results for Research Question 1a: Attendance Rate at Two Years Later (Post-Randomization) (n=135)

	Treatment	Control	Missing	Pooled Std Deviation	Hedges g	Cox's d
Child Welfare Data						
Gender (% Female)	46.25%	49.09%	0.00%			0.07
Race/Ethnicity (% Non-Hispanic White)	43.75%	49.09%	0.00%			0.13
Foster Care Placement Prior Year (Yes)	61.25%	61.82%	0.00%			0.01
Education Data						
Special Education Ever (Yes)	28.75%	23.64%	0.00%			0.16
Attendance	76.66%	80.76%	14.07%	0.24	0.17	

Table B-3. Baseline Equivalence Results for Research Question 1b: Course Passing Rate at One Year Later (Post-Randomization) (n=217)

	Treatment	Control	Missing	Pooled Std Deviation	Hedges g	Cox's d
Child Welfare Data						
Gender (% Female)	49.30%	52.00%	0.00%			0.07
Race/Ethnicity (% Non-Hispanic White)	43.66%	48.00%	0.00%			0.11
Foster Care Placement Prior Year (Yes)	61.97%	61.33%	0.00%			0.02
Education Data						
Special Education Ever (Yes)	30.28%	22.67%	0.00%			0.24
Course Pass Rate	71.96%	76.60%	29.03%	0.33	0.14	

Table B-4. Baseline Equivalence Results for Research Question 1b: Course Passing Rate at Two Years Later (Post-Randomization) (n=140)

	Treatment	Control	Missing	Pooled Std Deviation	Hedges g	Cox's d
Child Welfare Data						
Gender (% Female)	45.24%	48.21%	0.00%			0.07
Race/Ethnicity (% Non-Hispanic White)	44.05%	50.00%	0.00%			0.14
Foster Care Placement Prior Year (Yes)	61.90%	62.50%	0.00%			0.02
Education Data						
Special Education Ever (Yes)	29.76%	23.21%	0.00%			0.20
Course Pass Rate	71.35%	76.17%	30.71%	0.33	0.14	

Table B-5. Baseline Equivalence Results for Research Question 1c and 1d: Suspension Incidents Rate at One Year Later (Post-Randomization) (n=216)

	Treatment	Control	Missing	Pooled Std Deviation	Hedges g	Cox's d
Child Welfare Data						
Gender (% Female)	49.29%	51.32%	0.00%			0.05
Race/Ethnicity (% Non-Hispanic White)	43.57%	48.68%	0.00%			0.13
Foster Care Placement Prior Year (Yes)	62.14%	60.05%	0.00%			0.04
Education Data						
Special Education Ever (Yes)	30.71%	23.68%	0.00%			0.22
Suspended at All (Yes)	14.53%	20.00%	25.46%			0.23
Number of Suspension Incidents	0.20	0.77	25.46%	0.65	0.21	

Table B-6. Baseline Equivalence Results for Research Question 1c and 1d: Suspension Incidents Rate at Two Years Later (Post-Randomization) (n=134)

	Treatment	Control	Missing	Pooled Std Deviation	Hedges g	Cox's d
Child Welfare Data						
Gender (% Female)	45.57%	49.09%	0.00%			0.09
Race/Ethnicity (% Non-Hispanic White)	45.57%	49.09%	0.00%			0.09
Foster Care Placement Prior Year (Yes)	63.29%	61.82%	0.00%			0.04
Education Data						
Special Education Ever (Yes)	30.38%	23.64%	0.00%			0.21
Suspended at All (Yes)	12.31%	12.90%	29.10%			0.03
Number of Suspension Incidents	0.14	0.26	29.10%	0.58	0.20	

Table B-7. Baseline Equivalence Results for Research Question 1e: On Track to Graduation Rate at One Year Later (Post-Randomization) (n=95)

	Treatment	Control	Missing	Pooled Std Deviation	Hedges g	Cox's d
Child Welfare Data						
Gender (% Female)	57.37%	50.00%	0.00%			0.18
Race/Ethnicity (% Non-Hispanic White)	40.98%	44.12%	0.00%			0.07
Foster Care Placement Prior Year (Yes)	68.85%	61.77%	0.00%			0.19
Education Data						
Special Education Ever (Yes)	24.59%	23.53%	0.00%			0.04
Course Pass Rate	63.62%	74.54%	30.53%	0.36	0.30	

Note: Course Pass Rate baseline data used for these contrasts (Research Question 1e at one- and two-years post-randomization). All high school students with On Track to Graduation data have Course Passing Rate data, but not all students with Course Passing Rate data have On Track to Graduation data.

Table B-8. Baseline Equivalence Results for Research Question 1e: On Track to Graduation Rate at Two Years Later (Post-Randomization) (n=68)

	Treatment	Control	Missing	Pooled Std Deviation	Hedges g	Cox's d
Child Welfare Data						
Gender (% Female)	48.84%	56.00%	0.00%			0.17
Race/Ethnicity (% Non-Hispanic White)	51.16%	48.00%	0.00%			0.08
Foster Care Placement Prior Year (Yes)	62.79 %	56.00%	0.00%			0.17
Education Data						
Special Education Ever (Yes)	25.58%	28.00%	0.00%			0.08
Course Pass Rate	63.68%	67.06%	23.53%	0.34	0.10	

Endnotes

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